ART. XII.—*On Contagion.* By SAMUEL HENRY DICKSON, M. D., Prof. of Theory and Practice of Medicine in the University of the city of New York.

THE discussions upon this important and interesting topic have been of late assuming a more and more definite form, and we are now better prepared than at any former period for the consideration of the various doctrines which have been proposed and maintained in reference to it.

There are two essential elements which go to the description of this source of disease; first, that it should be germinal, that is, self-multiplying, reproductive; and, second, that this reproduction should depend upon, or be favoured by the very processes of disease which itself gives rise to.

The first of these propositions is undenied, and therefore need not be argued. A word has been coined to express this characteristic property of certain diseases by the Registrar-General of England, and is employed in his annual reports—"Zymotic;" the etymology of which, indicating an analogy with fermentation, might be imagined to intend a leaning to the opinion, that the organic germ of the affections thus classed is of vegetable character. But the phrase is loosely applied, including some endemics of which we cannot affirm the self-multiplication, and excluding many contagions, all indeed that are not febrile. The several modes of reproduction will be spoken of hereafter.

In the present state of our knowledge of the history of contagion, we cannot venture to declare it to be the *exclusive cause* of contagious diseases. All such diseases must have had a commencement independently of such cause, the very existence of which is secondary and derivative in the nature of things. Nay, some contagious diseases begin now-a-days, under our own eyes, spontaneously, or without perceptible connection with any source of contagious matter; as typhus, which none will doubt to be generated by filth and want of air, and low living; and psora, which will always arise among any very unelcanly collection of people who are supplied with food and fresh air enough to retain their general health; and gonorrhœa, produced by a more local form of impurity; and ophthalmia, developed by a certain concurrence of circumstances in Egypt often, or constantly, and elsewhere occasionally; and plague, a denizen of the same region; and yellow fever, indigenous in some American cities, and upon the coast of Africa.

The contingencies which give rise to typhus, psora, gonorrhœa, ophthalmia, pestis and yellow fever, may bring them into existence either directly or indirectly; *directly*, by exciting the tissues of the body into the morbid actions in which these diseases consist; *indirectly*, by first generating the matter of contagion. The former seems to me much the most probable. Life, whether a principle, property, or condition, is known to us only as *derivative* or secondary. "La vie ne nait que de la vie," the axiom of Cuvier, is absolutely true, Crosse's Electrical Acarus, and Weekes' Electrical Fungus to the contrary notwith-

standing. It is not necessarily transmitted through an ovum, however, as Harvey laid down. Dead animal and vegetable matter assumes life by mere admixture, and interposition with living fluids and solids after digestion and absorption. Organisms of various form, cells and granules, receive life from other organisms in which life abounds, or from which it is transferred to them under an infinite diversity of contingencies of disease, and decomposition and decay. This transference of life we see hourly all around us; in the presence of morbid parasites, animal and vegetable; in the masses of animal and vegetable life produced during animal and vegetable decomposition; and in the infinite abundance of infusoria wherever such decomposition can even be suspected. But the matter of contagion must be living matter, must be organized, however obscurely or imperfectly; and in whatever character or form, whether simply cellular or of complicated structure, whether a fungous sporule or an animalcule, its capacity of self-multiplication, of indefinite reproduction, necessarily implies its vitality. Nothing inorganic or inanimate can propagate itself.

The facts that go to prove its germinal character are clear, abundant, and familiar to every one. If its presence were only one among several and varied circumstances, concurring in the production of a given disease, its absolute sufficiency, or exclusive efficiency, might be questioned; but we have numerous examples in which all the concurrent contingencies may be thoroughly changed, or entirely got rid of, as in inoculation with vaccine matter, small-pox, or gonorrhœa, or syphilitic pus; and yet the result be regular and uniform. Hence, it is absolutely necessary to the definition of contagion, that each of its forms shall develop, or tend to develop, a materies morbi reproductive and self-multiplying; and propagating itself through, and by means of, diseased processes similar to, and identical with, those from whence it sprung.

The matter of contagion, as presented to us *palpably*, has undergone close examination and repeated experiment. In small-pox and vaccine, it exists in the limpid matter of the vesicle, lingers in the pus formed during maturation, and is still potential in the dried crust or scab. The foul fluids upon the surface of a chancre; within a syphilitic bubo, and the bubo of the plague; upon the conjunctiva in Egyptian ophthalmia, and in the gonorrhœal urethra; in an itch pimple, and a herpetic vesicle; all contain it, and admit of desiccation, without the loss of its characteristic and very peculiar properties.

"All fixed contagions," says Berres, "are, at their origin, alike in form, consisting of larger or smaller globules; we have no means of explaining the extreme variety of their effects and operations. We must allow a specific life and separate existence to contagion." Two assertions are made here, which should not be hastily received as established truths. I am not sure that the palpable matter of contagion has been actually seen in an insulated state, though the author just quoted describes it minutely. "Moist contagious matter," he says, "is a vesicle $\frac{1}{5000}$ of an inch in diameter with no trace of

organization; the dry is in the shape of scales, scurf, or abrasions; its essence consists in an aggregation of semi-transparent, grayish-white globules $\frac{1}{10,000}$ of an inch in diameter." But if we reflect that the matter of contagion is, in certain cases, diffused in the blood, as well as mingled with the limpid fluid of a vesicle, and combined with pus, not to speak in anticipation of its impalpable condition, we shall scarcely venture to conclude that we have detected or reached, and insulated it. The fluid of the vesicle contains it; it exists in the pus globule and in the cancer cell; but the cancer cell, the pus globule, and the lymph of the vesicle, are composed of many elements, and do not consist of contagious matter only. Chemical analysis has shown nothing peculiar in either of them. Lassaigne's analysis of small-pox pus exhibits nothing new or characteristic; presenting only a larger proportion of saline ingredients than is found in other pus. Like its composition, the form and appearance of contagious matter are unknown. Its minuteness is ultra microscopic.

Nor can we admit without farther discussion, the assertion made so distinctly that it enjoys "a specific life and separate existence," in other words, a sort of parasitic condition. Vogel declares "the question, What are parasites? is not yet ripe for decision." How are we to distinguish separate existence—true individuality? Is there any test that we can apply with definiteness and certainty?

Houston tells us "that the microscope shows particles, floating in the blood of definite and uniform shape, endowed with a veritable organization, individually alive, and forming a living mass. The white globules are fruitful in the production of granules, which granules are themselves, each and all, possessed of an independent life."

Addison maintains "the existence of active molecules" (the granules of Houston) in the interior of the cells of blood, saliva, and pus, to have become an established physiological fact." But Williams expresses a reasonable doubt whether motion constitutes a proof of individual vitality as here inferred.

The spermatozoa are treated of by Owen as members of the class entozoa, though he admits, "it is still undetermined whether they are to be regarded as independent organisms." These bodies not only present the phenomena of apparently spontaneous motion, like the blood globules above mentioned, and the moving filaments of the pollen of plants, and the inanimate particles enumerated by Williams; but they are reproductive probably. "There is reason," says Owen, "to suspect that they are oviparous; and they are also stated to propagate by spontaneous fission."

What shall we say of the hydatid, so frequently ranked among parasites? Owen, who recognizes it as "an independent organized being," denies it a place in the animal kingdom. But its chemical composition seems to forbid us to arrange it among vegetables; and indeed Lallemand says that he has seen voluntary motion in the human acephalocyst. Its reproduction by gemmules is familiarly known. Tubercle, too, looked upon doubtfully as a parasite by

Laennec, has been propagated, like hydatids, by Klencke, by inoculation. The cancer cell, which this author calls "semi-iudividual," both he and Langenbeck have propagated in that way. Is there then an intermediate condition, expressed as above, by the phrase "semi-individuality," in which we must place blood globules, spermatozoa, acephalocysts, tubercle, and cancer cells? For my own part, I regard the term as happily chosen, and, with the exception of tubercle, which is a morbid product characterized by no vital property whatever, I would consider all the bodies mentioned as endowed with a derivative life, not an indepeudent oue, but capable of transfer and multiplication. But the primary form of all life, which, according to Schwann and Schleiden, is cellular, is thus derivative and dependent. The lower we descend in the scale of vitality, the more transferable we find it. Thus vegetable gemmules live readily after inoculation or grafting; aud thus some of the lower order of germs found in and upon the animal body, whether themselves animal or vegetable we know not yet, as the hydatid and cancer cell, and some fixed contagions. The property of reproduction belongs to all these; but it has never been alleged to attach itself to tubercle.

We have abundant proof that the matter of contagion exists diffused in the blood. Scarlatina and rubeola have been conveyed by inoculation with this fluid. Small-pox not unfrequently affects the fœtus in utero; nay, there is a case on record, in which the child was attacked in the womb of a mother, exposed to the infection, but enjoying the immunity of a previous attack. It is a curious question, why this impregnation of the circulating fluid does not more generally occur in pregnancy. It would seem closely analagous with the conveyance of disease by transfusion in Coleman's experiments, and with the development of scrofula in the fœtus, as in the instances given by Lloyd.

A similar diffusion of contagious matter in secreted fluids, is presented in two examples at least. The hydrophobic saliva undergoes this poisonous change in the canine and feline tribes; whether in the human subject, is not clearly ascertained. Thus also inoculation with the tears of a patient labouring under rubeola, is said to be efficient in communicating the disease. The same thing is affirmed to be true of the mucus from the nostril; but this could hardly be procured free from some admixture from the tears trickling into it.

In some of the fixed or palpable contagious matters, we find animalculæ, which therefore, have been regarded as the cause of the disease, with which their presence is coincident; nay, as the very materies morbi. But we must not be hasty in drawing this conclusion. All diseases, or almost all, have been at one period or another ascribed to animalcular annoyance, and the question in regard to some of them still remains as doubtful as it is interesting. In some instances, the uniformity of the presence of animal life is denied. Dysentery was by Linnæus ascribed to an acarus, which few have been able to find. Klencke traces a connection between periodical attacks of vertigo and animalculæ in the blood; but he stands alone in this opinion. Reese saw animalculæ in recent black vomit; but no one has since repeated the

observation. Beauperthuis and Roseville assert that they have uniformly found animalculæ in cancer, to which they ascribe the disease, "erroneously," says Vogel, "even allowing that infusoria do sometimes occur as incidental parasites in cancerous ulcers." Donné and Ehrenberg describe a parasitic animalcule, an acarus or a trichomonas vaginalis, as inhabiting the vaginal mucus in syphilis. But Gluge, Valentin, and Vogel, regard the minute objects thus denoted as mere debris of uterine epithelium. And even when the uniform presence of the animalcule is undoubted, it may still be the effect and not the cause of the disease with which it is thus coincident. "The parasite," says Vogel, "should never be identified with the disease itself." Yet Vogel unhesitatingly ascribes psora to the itch insect. But psora is not only communicable by contact, but by fomites, upon which the exuding fluid has been long dry, as upon clothing, bedding, gloves, &c. It seems too much to assume, without proof, that the vitality of the minute acarus is retainable throughout an indefinite period of exsiccation, and that he may revive when moistened, ready for all the complicated functions of his peculiar mode of existence. Thus, also, certain vibriones being found in chancre pus, Donné regards them as the cause of lues venera; but Ricord denies this, upon the sufficient ground that bubo pus, containing no vibriones, is also contagious. An experiment of pretty conclusive character, might, I think, be made in psora. As the insect is not always found in the fluid of the itch vesicle, it might be used for inoculation, after careful examination with the microscope. The transfer of the insect taken from the fluid, would not be a test so satisfactory; as his body could not be perfectly cleansed from the fluid in which he lives immersed.

In certain contagious affections we discover the presence of parasitic vegetation, and this with great uniformity; as in tinea favosa, porrigo, and mentagra. The vegetable life here is always of the very lowest order, and is classed among the fungi. The same doubt exists here, as to the nature of the relation between the phenomena. Even by inoculation it is impossible to decide the question; because, from the extreme minuteness of these productions, it is impossible to transplant the vegetable, without some of the diseased crust or dried fluid in which it has grown and fixed itself.

We have now noted the several modes of alleged existence of the palpable matter of contagion. It is presented to us, 1. In the cell form. Langenbeck and Klencke have repeatedly conveyed cancer by inoculation with the cancer cell.

2. In the newly produced fluids of the exanthemata: as in the lymph and pus of vaccine, variola, and varicella.

3. In the pus globule of syphilis, of gonorrhœa, of Egyptian ophthalmia, and of plague bubo.

4. In the ichorous exudation of herpes, of hospital gangrene, and it would seem of erysipelas also.

5. In connection with the presence of animalculæ, as, especially, in psora,

less certainly in chancre pus, and, as Velpeau believes, in hospital gangrene, mentioned above.

6. In connection with fungous vegetation, as in tinea and porrigo, mentagra, and plica polonica.

7. In the blood, diffused as in scarlatina, measles, and small-pox.

8. In some of the normal secretions, rendered contagious by this morbid admixture, as in measles and hydrophobia.

I have already acknowledged that, although we thus apprehend and possess ourselves of the matter of contagion in these several modes, we have never yet separated or insulated it, so as to ascertain its external appearance and qualities, or detect its elementary composition or constitution in any instance. It has not been reached either by our best microscopes or our nicest chemical analysis.

In addition to what I have already said of its specific life, its parasitic character, so boldly affirmed by some theorists, I remark, briefly, that of the known parasites, both animal and vegetable, it is not proved that they are, in any well settled instance, the causes of the diseased condition with which their presence is coincident. Some parasites are not morbid in any sense whatever. Look at the bark of a tree in a forest, or an orchard, and see how it is beset and mottled with lichens or mosses. The live oak of the south, the solemn druid of the woods, flourishes mantled in the flowing drapery of the tillandsia usneoides, the Spanish graybeard, as it is sometimes termed. Animals also are infested with dependent animal life, both within and without. Now, no one imagines that in their ordinary number the ectozoa are at all injurious to health. Wilson, after Simon, describes a cutaneous acarus, inhabiting the oil tubes of the skin, as not only harmless but useful, "by stimulating these tubes to get rid of any undue accumulation of their contents."

Among the entozoa—which, as some naturalists maintain, are intruders from without, not natives of the regions they occupy, but immigrants from the external world—the most frequent and familiar to us, the intestinal worms, are only injurious to us incidentally, or, as it were, mechanically, by their undue amount. Nay, Parr and Rush maintain that they are appendages of a sound constitution, and aid in preserving its healthy state, acting as scavengers, or in some other way.

It is not by any means established that the fungi found on diseased surfaces cause the morbid condition with which they are associated. Vogel declares, that "they do not in general develop themselves upon mucous membranes, until, by morbid processes, a deposit of coagulated fibrine, which serves as a bed, has been prepared for them, and until this exudation has passed into a state of putrid decomposition." Yet it is not easy to perceive how this can be true of the fungi found in the aphthous affections of young children; nor can I imagine "a decomposing exudation within the sheath of the hair in mentagra," or in the interior of the hair roots in herpes tonsillaris and plica polonica.

What, then, is the relation of parasitic life to contagion? Shall we say with Vogel, that "the parasite should never be identified with the disease," or conclude with Prof. Mitchell, that "many diseases are proved to be dependent upon or arise from" parasites? The first-named author has made a distinction between parasites and pseudo-parasites, entirely unsatisfactory and indeterminate. The pathological interest of this branch of natural history lies in the assumed or discovered connection between the presence of animal or vegetable life and the symptoms attendant. Where no regular effect follows their presence, and it cannot be known or detected but by accident, our interest in the inquiry ceases. We cannot conjecture the existence of trichina spiralis, nor of the acephalocyst, nor the spiroptera, nor even of the strongylus—that is to say, there are no phenomena known to us as produced by them, or attendant specifically or otherwise on their presence. We do know pretty conclusively when a tenia resides in the digestive tube, though not, perhaps, with absolute certainty; so we may affirm also of the ascaris and lumbricus; the filaria is easily discovered; we infer the presence of the itch insect in psora readily enough, and of vibriones in chancre, and fungi in mentagra and porrigo, though we may not have seen them with our lenses. Perhaps it is not safe to say, in the present state of this department of science, that the former are accidental—do not belong to us—are merely temporary intruders when found; but we surely find a natural and definite line of demarkation between such forms of animal and vegetable life as exist or are noted elsewhere and such as are *peculiar to the human* economy, and *never are* or have been seen *anywhere else.* These last are, in every sense, *true*—the former, *pseudo*-parasites, in human pathology. Farther: if the absolute co-existence of certain specific morbid conditions, which never occur without and are never wanting to the presence of any definite form of parasitic life, be made out, and if these morbid conditions are communicable to healthy bodies by contact or near approach, it is illogical and unreasonable to refuse to consider the animalcule or fungus present as virtually the materies morbi. It finds in the diseased body its nidus, its soil, its local habitat; its transference to a healthy body is followed by the production of such disease as fosters it and propagates and multiplies it infinitely.

Whatever be the precise nature of a cell, whether animal or vegetable, whether absolutely an independent vitality, or a semi-individual, if it be never found but in the animal body, if always found in connection with and coincident to some special form of disease, and if such disease be regularly produced when such a cell has been introduced into the organism, then it is a *contagious cell;* it contains the materies morbi. And so of animalculæ and fungi; if found only in the animal body, if uniformly coincident with special diseases, which cannot or do not exist without their presence, and follow them when transferred, they convey the contagious principle—they are virtually the materies morbi. It is Liebig's idea, that, in examples of successful inoculation, a sort of ferment takes place in the fluids; but fermentation is nothing but

vegetable increase and germination. The analogy, however, is general. Each example is peculiar, separate, distinct. In every contagious morbid poison some new result has followed the combination of the elements which go to constitute it, which the highest magnifying powers of our best microscopes have not shown us, nor our nicest chemical analyses prevailed to detect, any more than they have made manifest the odorous particles of musk or of the rose.

If this be true of the fixed and palpable contagions, what shall we say of the tenuity of those which are designated as *impalpable*, which offer to us nothing tangible, but confound us by their invisible potency, and evade us by their independence of all definite modes of limitation?

Variola, as everybody knows, is not only conveyed from a diseased to a healthy body, by the insertion into a wound of a portion of lymph or pus, or dried scab, from a pustule, but is efficiently active *at a distance*—a healthy body being infected in the neighbourhood of a diseased one. Now we must ascribe the phenomena here, either to the radiation and diffusion of the same materies morbi which is contained in the scab, pus, and lymph, and which, mingled with the blood of the mother, passes through the placental tissues and infects the fœtus in utero, or we must assume the production and elimination of two efficient forms of contagious matter, the one fixed and the other volatile. But the first I have already proved to be ultra microscopic in the minuteness of the atoms which constitute it in certain cases, and, therefore, I see no difficulty in its elimination and radiation from the diseased body. It is, perhaps, relevant to observe, that this remote infection does not occur in any instance in which the parasite, whether animalcular or fungous, is visible, or has been detected by the microscope—it is chiefly confined to the febrile class of contagious diseases, and, perhaps, belongs exclusively to that class; but as there is yet some dispute as to the febrile, or non-febrile, character of certain epidemics, I abstain from announcing the rule as established. The portion of atmosphere about the person of a small-pox patient becomes thus charged with contagious atoms. But, like all other zymotic diseases, small-pox, sporadic and perennial in great cities, becomes, from time to time, epidemic—that is, widely and pestilentially prevalent. Healthy bodies are now infected in great numbers, most obscurely, and without known approach to a diseased body—the atmosphere is filled with the materies morbi. In the winter of 1847–48, I attended forty patients attacked with small-pox, not one of whom had ever seen a case, or consciously visited an infected neighbourhood. Now we must conclude, either that the contagious matter is diffused in prodigious abundance from its human sources, or that some other source, atmospheric or telluric, exists, from which the disease or its cause may spring.

But the spontaneous production of small-pox is contended for by no one. In this field of hypothesis, I do not hesitate to offer the suggestion, that in the animal exhalations collected in the dense population of a crowded city, the organic germs, fungous or animalcular—vegetable or animal, rather—may

find occasionally all the elements essential to their germination and growth; and may multiply and propagate themselves in an atmosphere thus filled with animal matter adapted to their support and development. A bulb, or a seed, which ordinarily requires soil to vegetate in, will, nevertheless, grow if laid upon a surface which admits of the moistening of its roots, as we see every day in the flourishing of rice laid upon damp cottou, and the budding of a hyacinth in a glass bulb filled with water. Nay, the numerous *air plants* furnish us with a still closer analogy; and many of the lower classes of minute insects will produce several generations in succession without access to their special food or habitat, as the *aphis* and the common *tick* of our southern country. Thus, then, whether we regard the materies morbi of contagious epidemics as fungous or animalcular, there is no difficulty in comprehending their propagation and extension, widely, actively, promptly, in dense populations and crowded cities; favored always, as we notice, by heat, moisture, and atmospheric stillness; repressed, more or less, by cold, dryness, and ventilation, winds, or dilution of air. There is no more difficulty in explaining why a sporadic pestilence should be only occasionally epidemic, than in pointing out the causes which make one season prolific in familiar fruits and insects, and another as remarkable for their comparative scarcity.

I have referred specially to small-pox; but the same things are equally true of all the contagious diseases of which fever is an essential part, with the single exception of vaccine, which is incapable of affecting a sound body unless admitted by a wound—and which gives out no radiation, no impalpable contagious matter. We must not forget, however, that it is not a human disease. Like glanders and hydrophobia, it is generated among the lower classes of animals, and, like them, it may be contagious by exhalation, or even epidemic, among its proper subjects.

I have already mentioned the fact, that none of the non-febrile contagions are impalpable or diffusable in the air. All require contact. Even the itch insect does not travel; at least, he has never been met with on a journey.

One great obstacle in the remaining steps of our inquiry, is the uncertainty how far we may allow ourselves to be led on by analogy. Variola shows us a palpable contagious matter, and also, undeniably, spreads abroad an impalpable contagion; but no two contagious diseases present exactly similar phenomena. Scarlatina and rubeola have been communicated, we are told, by inoculation with blood, and the latter by means of tears. But we have now entered the domains of doubt and dispute, and as we shall find the mass of physicians denying the contagiousness of certain diseases, because no fixed mode of contagious matter has been detected in them, so, on the other hand, Chapman and Bell express their skepticism as to the existence of any palpable contagion in measles and scarlet fever, and prefer to attribute the infection of those inoculated by Home, Speranza, and Von Katona, to an epidemic diffusion of the impalpable materies morbi in the surrounding air. Following the general belief, however, as it is my own, I enumerate, as manifesting both

the palpable and impalpable modes of contagious infection, pestis, erysipelas, hospital gangrene, and varicella, in addition to those above mentioned. I do not pretend to make the list complete, but wish merely to give varied examples.

There are, besides these, certain diseases which have afforded room for almost interminable and angry discussion as to their contagious or non-contagious character. These exhibit no fixed or palpable materies morbi; all attempts to convey them from a diseased to a healthy body, by any mode of inoculation, has failed; there is much irregularity in the proportion of infections by near approach; and yet they are regarded, properly, as contagions, because they are zymotic, rapidly and widely becoming epidemic, self-multiplying, and easily propagated in dense populations and crowded cities—one case often serving as a focus around which is radiated a poison, intense and abundant enough to infect thousands with a morbid condition precisely identical. Such is typhus; such pertussis; such puerperal fever; such dysentery; such yellow fever; such cholera asphyxia. Each of these has been the topic of vehement dispute; but time, which affords the best touchstone of truth, has gradually accumulated facts enough to satisfy the more observing and philosophical authorities in our science; and the period is at hand when all specific diseases capable of epidemic diffusion, that is, unconfined by any exclusive local limitation, shall be received into the same category, and pronounced contagious.

Of these, I hold that they differ less than is generally supposed from the acknowledged contagious affections. It is true, that the materies morbi is not detained or fixed in any of the fluids of the body, or disposed to collect itself in any cell or globule; nor does it assume any shape visible or tangible. But even in the so-called palpable contagions, I do not admit that we have detected the form, or outline, or composition of the contagious germ. With very few exceptions, already specified—with very few exceptions, and these, perhaps, admitting of some doubts—whether a cell or a sporule; whether animal or vegetable; whether mixed in blood, lymph, pus, tears, or saliva, it is ultra-microscopically minute and imperceptible. And in the febrile contagions we have examples of contagious matter unfixed, undetected, indescribably tenuious, irregular, and entirely uncertain as to its limit and potency—as in variola, scarlatina, rubeola, and pestis.

That the materies morbi in the disputed instances under consideration is, as in all other zymotic diseases properly so called, an organic germ capable of self-multiplication and reproduction, surely needs no proof. Otherwise we could not account for their extension and propagation, their absolute freedom from exclusiveness as to locality, their attendance upon migratory masses, their close adherence sporadically to well adapted foci, and their prompt multiplication under fostering contingencies. These are not modes of existence, or laws of causative action of any known inorganic elements.

That it finds a necessary nidus, centre, or habitat in the human body, is

also evident from the fact that these diseases only prevail where there is crowded human life, whether in dense cities, or camps, or emigrant ships, or caravans; and that their virulence is always, *ceteris paribus*, directly proportioned to the density of population.

There is no reason for our believing that contagion is exclusively animal or vegetable in its character. In different examples it may be of different nature. In either case the organic germs are liable to be repressed or fostered in their growth by contingencies known or unknown to us, the known having been already alluded to. In either case, they are capable of sudden multiplication to an unimaginable extent under favouring circumstances. I need not go into any statement of the possible increase of animalcular life, or the prodigious augmentation of masses of fungi—millions of each being generated in a few hours from a single individual, and the increase going on in an infinitely accelerated ratio.

None of the hypotheses offered to enlighten and guide us in these our obscure inquiries are free from difficulties, it is true; but the skeptics who deny the organic nature of the cause of these zymotic diseases, have chosen to contend with insurmountable obstacles. Let us select the example of Asiatic Cholera. It is so widely prevalent, that immense masses of the causative poison which excites it must exist on the earth's surface. This poison must either be generated wherever it is active, or it must have been produced originally in infinite abundance to admit of its effective diffusion over such vast spaces. The first notion is most generally entertained; but nothing can be imagined more contrary to all experience or analogy than the telluric elimination, or the atmospheric production of the same inorganic, chemical, or mechanical poisonous agent—call it malaria, or what you will—in the depth of a Russian winter at Moscow, in the burning summer heat of Hindostan, in the autumnal vicissitudes of weather at New Orleans, and in the changeable spring of the Rio Grande and the Ohio; bidding defiance, alike, to climate, and season, and temperature, and over-riding all local contingencies, all geographical and geological peculiarities; gushing from the alluvion of the Delta of the Ganges and the Mississippi, and the arid sands of the great Arabian Desert; hovering over the granite of Staten Island, and the gneiss of New York, and the coal formations and calcareous soils of the Great West; infesting alike the sea-coast and the shores of our fresh-water lakes, the banks of the Seine and the St. Lawrence, the Volga and the Thames—nay, exploding vehemently among the passengers of a solitary barque on the wide Atlantic, healthy until the moment of an abrupt fall of the atmospheric temperature.

If the other horn of the dilemma be chosen, little appears to be gained. Wandering veins of cholera atmosphere, coming we know not whence, and tending we know not whither, cross the track of ships far away on the ocean, or touch upon the coast at remote points, as at Folly Island and the quarantine ground at the outlet of the Hudson and at New Orleans, rebounding to sea again, or disappearing into the upper regions of air, or following the angles

and windings of navigable rivers, and the inextricable mazes of roads and highways.

It is curious, that some of those who admit and contend for the organic nature of the germs which constitute the materies morbi of certain forms of disease, should nevertheless broadly deny their contagious character. This is in each case a simple question as to the habitat of the germ, its favoring soil, its proper nidus, its source of life and nourishment. If these be in the human body—if, as Professor Mitchell expresses it, "the affinity is to the person rather than the place," surely the contagious character of the germ is decided. But it is not difficult to prove that, unless we except the immediate district around Jessore, there seems to be no special affinity to place, predicable of the cause of cholera. Its history shows it to be singularly migratory; and it is notable that no spontaneous infection with it has ever occurred in visiting any unpeopled region. Contrast it with malarious fever, remittent or intermittent, in this regard. Let a healthy man sleep in a remote rice field, a dismal jungle, or wild swamp, in a southern summer night; you may safely wager any odds that within three septenary periods he will be seized with disease of known type; but you can procure for such a one an attack of cholera no where but among subjects already labouring under the pestilence, or in their neighbourhood, or as infected by fomites which have been exposed upon or near the persons of the sick. The cause of cholera, unlike the cause of periodical fever, may be transported with persons anywhere; in no place does it remain fixed; it is now totally extinct in many places where it once abounded. In all this it closely resembles the cause of small-pox, which surely relates to person and not to place; which finds its pabulum in a certain proportion of healthy bodies, and dies out when it has consumed it, or marches with them or their fomites to prey upon new food in other healthy bodies.

ART. XIII.—*Contributions to Pathological Anatomy.* By JNO. NEILL, M. D., Demonstrator of Anatomy in the University of Pennsylvania. [With five wood cuts.]

CASE. I.—*An Oblique Fracture of both Condyles of the Femur united.*—This specimen was presented to me by Dr. Benedict, from whom I obtained the following history of the case.

An Irishman, of about 40 years of age, in attempting to escape from the third story of the Blockley Hospital, fell from a considerable height, and alighted upon his feet. Upon examining him immediately after the accident, the exact nature of the injury was not very evident. There was no crepitation, no twisting of the foot, no increased breadth of the knee, no difficulty in making passive flexion and extension of the leg. The deformity simulated that of a partial luxation of the tibia posteriorly; the leg was thrown backward and the patella very prominent.

The case was treated by Drs. Benedict and Page for fracture, by a long